

Interactive comment on “Age distribution, extractability, and stability of mineral-bound organic carbon in central European soils” by Marion Schrumpf et al.

Anonymous Referee #1

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The manuscript reports on experiments characterizing mineral associated organic matter using a comparison of "extraction" by NaOH-NaF versus "oxidation" by H₂O₂. The treatments were coupled with ¹³C-NMR and ¹⁴C dating. The experiments are well designed according to current paradigms of SOM stabilization, but the manuscript requires substantial revision before it might be acceptable for publication.

There are a large number of instances throughout the manuscript where diction and grammar are awkward or incorrect. The manuscript should be deeply reviewed by a native English writer to revise these subtleties.

The end of the introduction should be restructured to move ln96-104 to the end of the

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section, where it will serve as a segue to the methods section. That is, I recommend an introduction with the structure: background, problem statement, hypotheses, ways to test them.

The hypotheses listed are not hypotheses *sensu stricto* because they are not testable in the strictest way. That is, they cannot be answer with a simple "yes" or "no". Several hypotheses have multiple conditions or clauses that should be broken into several sub-hypotheses. I am not necessarily a purist when it comes to these formulations, but when I know this, I typically replace the word "hypothesis", which can be reserved for strict statistical or logical uses, with the word "expectation". All this being said, I do recommend trying to break up the 5 bullets into expectations and outcomes. That is, it appears that some of these so-called hypotheses are actually expectations, which if are true, other conditions would also be true. Others are more contradictory, where if not true then... A clearer, more explicit and deliberate structure for all these expectation will result in better structured results and discussion sections.

A few minor points in the methods section: In141: filters are normally described at least by their pore-size and sometimes their diameter, not their diameter only. In161-162: The sentence on carbonates is unnecessary and likely the result of copy-paste text because In158-159 already state that all soils were carbonate-free.

In some instances the order in which results are circuitous and confusing. Perhaps use the "hypotheses" or methods section as road maps for ordering the presentation of results.

I like how the discussion section is structured. It places the results in a clear context. However, I'm not convinced that the results are "surprising", and I find the overall interpretation to be a little off the mark. I did not find it surprising that the proportions (vs. "portions" which is incorrectly used throughout the manuscript) were consistent across most soils. This has been previously been observed for both H₂O₂ (eg, Plante 2014 EJSS) and acid hydrolysis (eg, Paul 2006 SSSAJ). This is one of the problems with

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chemical extractions: they rarely demonstrate the expected trends in inferred stability. A similar lack in expected trends has also been frequently observed in ^{14}C dates. So, given that a substantial proportion of the results from this study did not meet expectations, I would strongly recommend reframing the manuscript. It might be much more compelling to more specifically spell out what the conceptual framework (paradigm) is that leads to the expectations outlined. The goal of the discussion would then be to describe where the problems are with either the assumptions etc. in the conceptual framework, or in the methods used to test them. The manuscript currently tries to address the former, but not necessarily the latter. Are NaOH-NaF and H_2O_2 appropriate tools for probing SOM stability? OR is our conception of SOM stability incorrect? The match between expectations and results that I am referring to is well illustrated in the diction of the subheader in In344. The use of the word "Missing" suggests it was expected a priori. A more objective and unbiased approach would be to refer to it as "lack of".

I also found it unsurprising that the chemistry of extracted OC was similar across samples and dominated by polar molecules (eg, alkyl). In essence, the experiment demonstrated the solubility of a polar fraction of OM in a highly polar solution. It would not be a reasonable expectation to see non-polar OM (eg, aryl) in such a polar solution, or vice-versa.

The tables and figures used to report the results are appropriate, though the figures are numerous.

While it is appropriate to report the 1:1 line in Fig7 (modeled vs. measured), I'm not sure why it is reported in Fig3. It seems to me the slope represents the proportion extractable. If I had to guess, the slope would be 0.58 (or its inverse). I'm not sure what it would mean to have the data fall on the 1:1 line in this case.

I have become increasingly frustrated by the visual and qualitative interpretation of spectral data using a stack of "squiggly lines". While large differences might easily

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be apparent, smaller, more subtle differences, or large differences in smaller peaks may not be so apparent. Differences among spectra should be tested quantitatively/statistically, perhaps using a multivariate method such as PCA or NMDS.

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